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# Climate and Conflict: A Comment on Hsiang et al.'s Reply to Buhaug et al.

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# Climate and Conflict:

## A Comment on Hsiang et al.'s Reply to Buhaug et al.

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**Cover Photo:** The photo depicts members from the Landless Workers' Movement (Movimento dos Trabalhadores Sem Terra), one of the largest social movements in Brazil. Hidalgo et al. (2010), one of the included sample studies in the meta-analysis, study land occupations initiated by this movement.

## Introduction

Hsiang et al. (2014; henceforth HBM) discuss five alleged errors committed by Buhaug et al. (2014). We assess these claims and find that they largely miss the target. HBM are correct in pointing to a minor coding error and we also agree with HBM that Buhaug et al. could have been clearer in explaining some of their proposed modifications. Those issues notwithstanding, we conclude that Buhaug et al.'s concerns about the viability of the fixed effect-based meta-analysis presented in Hsiang et al. (2013) remain valid.

## On meta-analysis

The main disagreement between Hsiang et al. (2013) and Buhaug et al. (2014) concerns which parameter to rely on when using meta-analysis to make inferences about the general relationship between climate and conflict.<sup>1</sup> A brief discussion of some facets of meta-analysis is in order. As Gelman et al. (2014, p. 125) explain, the researcher may be faced with three kinds of samples:

The first possibility is that we view the studies as identical replications of each other, in the sense that we regard the individuals in all the studies as independent samples from a common population, with the same outcome and so on. A second possibility is that the studies are so different that the results of any one study provide no information about the results of any of the others. A third, more general, possibility is that we regard the studies as exchangeable but not necessarily either identical or completely unrelated.

The model normally used when one believes the data correspond to the first possibility is the so-called *fixed effect model*,<sup>2</sup> whereas a *random effects model* is appropriate if one believes the data resemble the third possibility (Borenstein 2011). The second possibility described by Gelman et al. does not have a model as it does not make sense to conduct a meta-analysis of disparate and unconnected studies.

Having chosen a random effects model, there are two different quantities one may sensibly report, the mean of the distribution of effect sizes,  $\mu$  (and its estimated standard error), and the predicted distribution of effects,  $\theta$ .  $\mu$  will have a wider standard error than the estimated mean effect in a fixed effect model because the random effects model takes into account between-study precision as well as within-study error. The wider the spread of effects in the meta-study sample, the more weight is put into the individual study estimates, while the larger the error in individual studies, the less weight is put into that individual estimate.

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<sup>1</sup> Buhaug et al. also raise a second concern; the likelihood of selection bias in Hsiang et al. (2013) by (i) suppressing studies of previously tested associations, (ii) only selecting one climate indicator from each sample study, and (iii) not accounting for variations in potential for generalization between studies. Since HBM did not respond to this concern, beyond conducting a hypothetical "stress test" based on the inappropriate fixed effect model, we do not revisit this point here.

<sup>2</sup> Not to be confused with fixed effects regression models, which may be used in individual sample studies.

If one is confident that the sample studies are estimating mostly the same effect, reporting  $\mu$  makes sense. If, on the other hand, one believes the effects are likely to be different (but not so different that a meta-analysis carries no meaning), one should report and interpret the more conservative  $\theta$ :

Uncertainty about the probable treatment effect in a particular population where a study has not been performed (or indeed in a previously studied population but with a slightly modified treatment) might be more reasonably represented by inference for a new study effect  $[\theta]$ , exchangeable with those for which studies have been performed, rather than for the overall mean  $[\mu]$ . (Gelman et al. 2014, pp. 127–8; terms in brackets added)

It should be clear that deciding on the model to estimate and the parameter(s) from the model to report depends on the assumptions about the underlying data, whether they represent one effect, more or less the same effect, or mostly different effects. This decision has fundamental implications for the goal of the meta-analysis: At one extreme (the fixed effect method), the goal is to estimate the true effect size – because one believes there is such a thing. At the other extreme (i.e., reporting the conservative  $\theta$  from a random effects model), the goal is to estimate the true distribution of effects that gave rise to our sample effects (or alternatively estimate the predicted effect of a hypothetical future study). If this distribution is approximately normal, then it is sensible to report the 2.5<sup>th</sup>, 50<sup>th</sup> (the median) and 97.5<sup>th</sup> percentile from the estimated distribution of the random effects model.

Against this backdrop, we assess the reply by HBM to Buhaug et al. We discuss the five alleged errors in the order they are presented in HBM.

## 1.

Buhaug et al.'s (2014) modified meta-analysis reports  $\theta$  from a random effects model (in this case, a Bayesian hierarchical model). HBM criticize this modeling choice and argue that they should have followed Hsiang et al. (2013) in reporting the estimated mean and confidence interval from a fixed effect model instead. However, as discussed above, the fixed effect approach rests on a strong assumption about causal homogeneity which clearly is violated in this case. In the words of Buhaug et al. (p. 3),

[the] sample of 'intergroup conflict' studies covers a wide range of social phenomena, from non-violent land grabbing via urban riots to major civil war; a wide range of climatic events, from heat waves via excess rainfall to global ENSO cycles; and a wide range of spatial scales, from municipalities via countries to the entire world.<sup>3</sup>

In other words, the data obviously are closer to the second than the first possibility outlined by Gelman et al. in the quote above, rendering the fixed effect model inappropriate. Relying on  $\mu$  and its standard error also requires confidence that the sample studies are measuring mostly the same causal effect. We lack such confidence and believe that Buhaug et al.'s

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<sup>3</sup> In this sense, HBM's (pp. 3–4) hypothetical example of how to estimate the mean age for a sample with 100 observations of a similar kind (individuals) with a known age distribution is misguided; the data considered in Hsiang et al. (2013) are nothing of that sort.

decision – to report  $\theta$  from a streamlined sample of civil conflict studies – is the most reasonable approach. Indeed, in their supplementary material (2013, p. 19), Hsiang et al. acknowledge that causal heterogeneity is a likely problem:

It is likely the case that differences among estimated effect sizes are not due to sampling variability alone. That is, studies looking at the effect of climate on different outcomes might be expected to share some similarities (different outcomes might be related), but also some important differences (some outcomes or samples might exhibit different responses to climate).

Hsiang et al. (supplementary material) go on to discuss the Bayesian hierarchical model and they also visualize  $\theta$  in the right panel of their Fig. 5.<sup>4</sup> However, they give undue emphasis to the fixed effect model when concluding on the strength and robustness of the general association between climate and intergroup conflict (i.e., they report an average effect of 11.1% ( $\pm$  2.62%) increase in intergroup conflict for each standard deviation increase in climate).

## 2.

HBM criticize Buhaug et al. of presenting a figure (Fig. 1) that is not directly comparable to Hsiang et al. (2013) as it is based on a model that is specified with all lagged effects instead of relying on some contemporaneous and some lagged effects as Hsiang et al. do. The reasoning behind Buhaug et al.'s modeling choice is partly to maximize analytical consistency and partly to ensure that the treatment occurs prior to the outcome. This is fully explained in Buhaug et al. (2014, p.4) and does not constitute an error but rather reflects disagreement with the model specification preferred by Hsiang et al.

However, we note that Buhaug et al. failed to explain why they excluded the result for Hsiang et al. (2011) from Fig. 1. The reason is again analytical consistency; The Hsiang et al. (2011) study considers a global ENSO effect whereas the studies in the valid sample investigate a local effect (i.e., the treatment is measured specifically for each unit of analysis). Moreover, Maystadt et al. (2013) was excluded because Buhaug et al. failed to obtain the original replication data for the (at the time) unpublished study and hence were unable to re-estimate their model with lagged climate indicators. This is stated in footnote 5 in Buhaug et al.'s supplementary information.

## 3.

HBM criticize Buhaug et al. for reclassifying some of the studies described in Hsiang et al. (2013). Specifically, Buhaug et al. exclude the effects of ENSO and PDSI when estimating the aggregate temperature effect. The reason is simple: ENSO and PDSI do not measure temperature. [PDSI](#) is a measure of drought; [ENSO](#) is a fluctuating climatological

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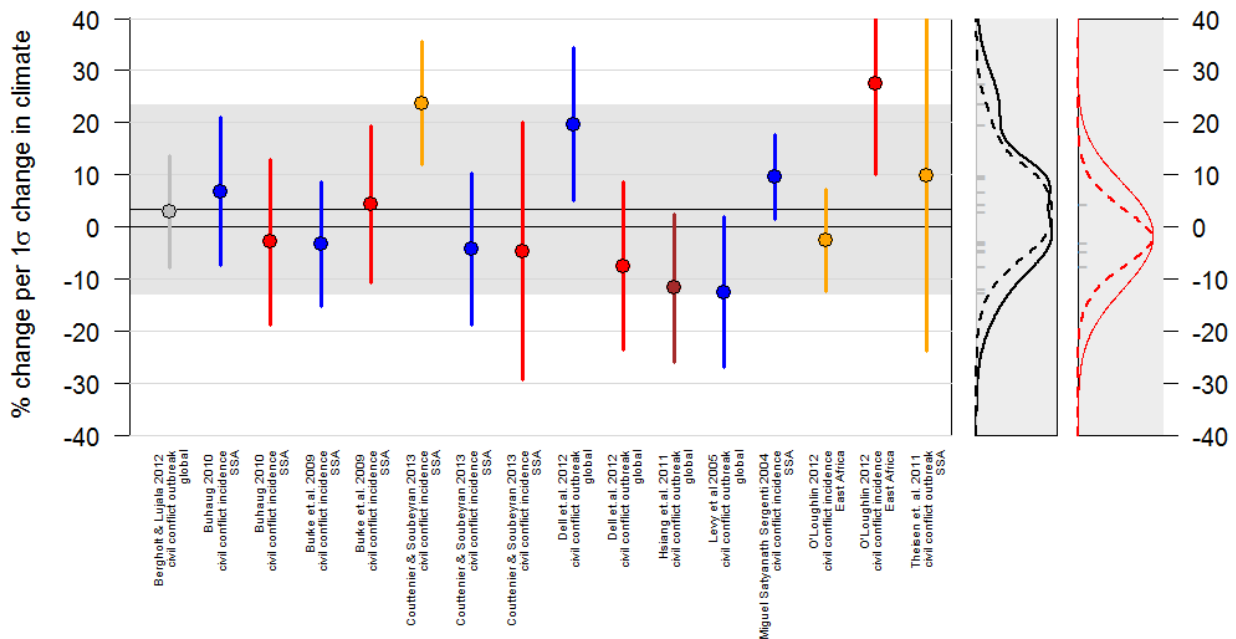
<sup>4</sup> To be precise, they estimate two slightly different distributions, one derived from a variance-weighted random effects model and one using the Bayesian hierarchical model.



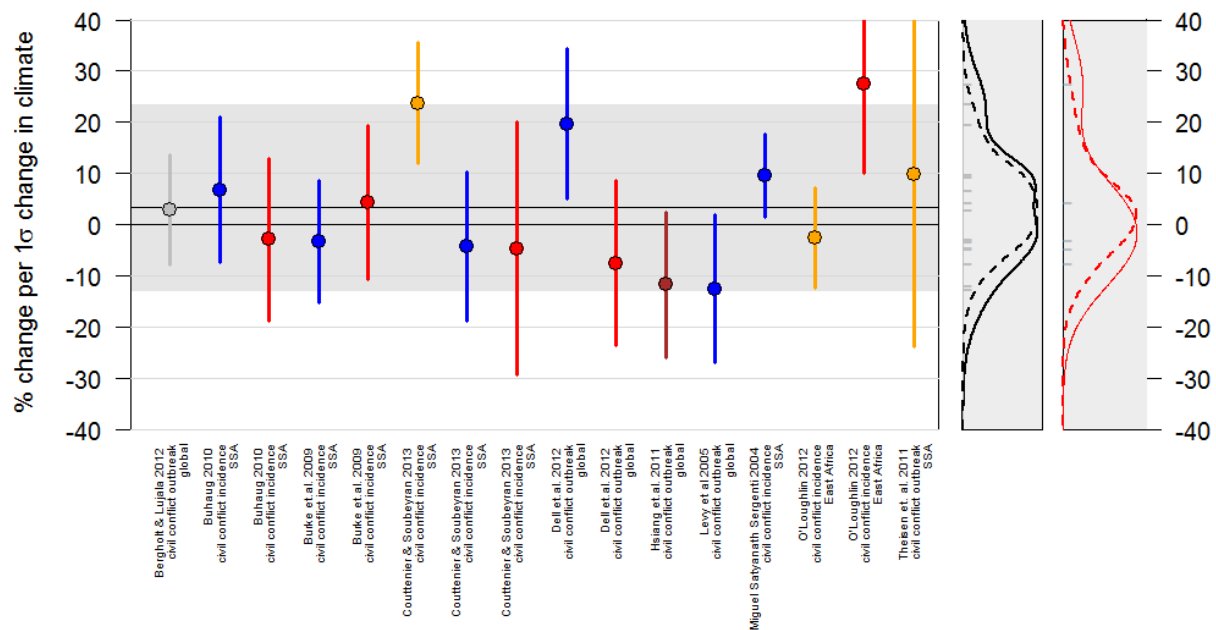
phenomenon associated with shifts in sea surface temperature and atmospheric pressure across the Equatorial Pacific Ocean, with different local manifestations. Similarly, Buhaug et al. decided to follow convention and classify the Standardized Precipitation Index (SPI) as an indicator of drought. While these alterations could have been better explained, we fail to see how Buhaug et al.'s more conventional classification constitutes an error.

#### 4.

HBM identify a coding error in Buhaug et al., whereby one of the two results from O'Loughlin et al. (2012) is unaccounted for when  $\theta$  for the aggregate temperature effect is estimated. This is a correct observation, and we thank HBM for pointing this out. A comparison of the original and corrected figures (Figs. A and B) reveals that this error does affect the shape of the distribution at higher values but has little influence on the interpretation of the overall temperature-conflict pattern



**Fig. A.** Original estimates for the effect of climate variability on civil conflict. By mistake, the temperature effect of O'Loughlin et al. (2012) – the rightmost red plot – was not accounted for when estimating the effect distribution for temperature (red curve). See Buhaug et al. (2013) Fig. 1 for further details.



**Fig. B.** Corrected estimates for the effect of climate variability on civil conflict.

## 5.

Lastly, HBM criticize Buhaug et al. for not visualizing the variance-weighted mean and confidence interval in their Fig. 1. This goes back to the discussion in Section 1 above. To reiterate, we believe the fixed effect-based estimation that Hsiang et al. (2013) apply is inappropriate in this setting given the large heterogeneity in the data sample. A visualization of the distribution of values from a random effects model (e.g., the Bayesian hierarchical model) and interpretation of the shape and range of the effects in our view make more sense since we lack a solid theoretical foundation to expect a single, common climate effect.

## Concluding remarks

We thank HBM for their reply, which allowed us to correct a minor coding error and provide a more detailed explanation of the concerns raised in Buhaug et al. (2014). In fact, we acknowledge that we may not have given Hsiang, Burke, and Miguel the credit they deserve for embarking on a truly innovative project, with the aim of providing a more transparent and rigorous quantification of the state-of-the-art on climate and conflict than extant qualitative literature reviews have been able to offer. We also appreciate their exemplary [replication material](#), which was easy to use and read.

So, where do we stand on this? The following brief statements may be illuminating:

- a) We agree that meta-analysis in principle can be a useful tool to synthesize and quantify results from the empirical climate-conflict literature – provided the researcher investigates comparable studies of similar phenomena and accounts for variations in representativeness and potential for generalization. This is why Buhaug et al.'s modified meta-analysis was run on a reduced sample, limited to civil conflicts (although one might credibly argue that the sample should have been streamlined further by also limiting focus to a single type of climate parameter and involving one type of actor only).
- b) We do not believe in a single, common climate effect that connects various distinct climatic conditions (drought, heatwave, excess rainfall, etc.) with various distinct forms of violent conflict (urban riots, land grabbing, insurgency, etc.) at various spatiotemporal scales in a direct and simple manner. Lack of a comprehensive theoretical framework, a demonstrably large variation in estimated effects across studies, and careful reading of the empirical literature (e.g., Adger et al. 2014; Klomp and Bulte 2013), also are at odds with the assumption of causal homogeneity.
- c) For reasons explained in b), we believe a meta-analysis of the empirical climate-conflict literature based on the variance-weighted fixed effect model, where the goal is to estimate the “true” climate effect, is inappropriate. We also believe the random effects-generated mean estimate ( $\mu$ ) in itself has limited value in this context since it, too, rests on quite strong assumptions with respect to causal homogeneity.
- d) We agree that the *mass of effects* in the complete pool of estimated effects of climate-related variables on civil conflict appears to be larger above zero than below zero; in other words, more than half of the area of  $\theta$  in Buhaug et al.'s Fig. 1 is located above zero. However, this is different from concluding on a sweeping, positive climate-conflict relationship; the between-study variation simply is too large, rendering reference to (and quantification of) a singular effect of little substantive meaning.
- e) We urge scientists to exercise caution when reflecting on the wider implications of this research. The tendency toward a larger mass of positive effects cannot be taken as evidence that sustained warming will increase civil conflict risk in the future. That question is not addressed in the surveyed literature, which studies effects of climatic variability, not climate change. It is not given that societies tomorrow will respond similarly to what will then be normal weather as societies today respond to climatic anomalies.

- f) We believe there is more to be said about the risk of selection bias than what we have commented on here. Specifically, decisions on exact operationalization of climate (and conflict), specification of functional form and time lag of the effect, and other modeling choices may be informed partly by the author's *post hoc* knowledge about their statistical implications. Sampling results based on what is reported in individual studies instead of specifying a coherent, theory-informed meta-analysis building on the original datasets may potentially accentuate such bias. This concern, as well as the related "file drawer problem", is of course not limited to climate and conflict, or to quantitative syntheses of that literature, but climate change-related research (and funding opportunities) may be subject to particularly powerful norms of conformity that reward certain results more than others (e.g., Lahsen 2013; see also Franco et al. 2014; Kicinski 2013; Rothstein et al. 2005; Rosenthal 1979; Schooler 2011).

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# Climate and Conflict: A Comment on Hsiang et al.'s Reply to Buhaug et al.

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Last year, a meta-analysis published in *Science* by Hsiang, Burke, and Miguel (HBM) argued that climate is robustly and causally related to many forms of conflict. A commentary article by Buhaug et al. questioned the assumptions and sample selection strategy underlying this analysis, and showed that these decisions had a dramatic impact on their conclusion. In a reply to the commentary, HBM dismiss these concerns and instead claim that Buhaug et al. committed five key errors in their own analysis. We assess these claims and find that they largely miss the target. HBM are correct in pointing to a minor coding error and we also agree with HBM that Buhaug et al. could have been clearer in explaining some of their proposed modifications. Those issues not-

withstanding, we argue that Buhaug et al.'s concerns about the viability of the fixed effect-based meta-analysis presented in Hsiang et al. (2013) remain valid.

While we agree that meta-analysis in principle can be a useful tool to synthesize results from the empirical climate-conflict literature, we find it implausible to assume a single “true” effect that connects various distinct climatic conditions with various distinct forms of conflict at various spatio-temporal scales in a simple and direct manner. For this reason, the estimated average effect that Hsiang et al. refer to has little substantive meaning. Instead, we find it useful to interpret the distribution of estimated effects across studies, whose variation is inconsistent with the claim of a robust aggregate climate-conflict relationship.

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